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TRANSACTIONS

OF THE

AMERICAN INSTITUTE

OF

ELECTRICAL ENGINEERS.

SPECIAL MEETING, NOVEMBER 18TH, 1890.

SUBJECTS:

Revised Report of the Standard Wiring Table Committee.

The Theory of Compound Winding for Constant Potential.

BY DR. LOUIS BELL.

A New Method of Analyzing Armature Reactions of Alternators.

BY CHARLES STEINMETZ.

List of New Associate Members.

ELECTED SEPT. 16th, OCT. 21st, NOV. 18th, and DEC. 16th.

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RALPH W. POPE, *Secretary*,

12 WEST THIRTY-FIRST STREET,

NEW YORK CITY.

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1890-91.

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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Discussion of the Revised Report of the Standard Wiring Table Committee.

The fifty-first meeting of the Institute was held at 12 West Thirty-first street, New York, November 18, 1890.

The meeting was called to order by the President, Prof. W. A. Anthony.

The President:—Have the Committee on the Standard Wiring Table anything to report to-night?

Mr. F. B Crocker:—In accordance with the instructions of the meeting of the Institute held two months ago, the "Standard Wiring Table Committee" have amended their report. The report as amended stands as follows:

REPORT OF THE STANDARD WIRING TABLE COMMITTEE.

Matthiessen's Standard of Resistance of Copper.

The Council of the American Institute of Electrical Engineers, at its regular monthly meeting held December 3d, 1889, appointed a Committee "To formulate and submit for approval a Standard Wiring Table for lighting and power purposes."

The Committee appointed consists of the following members of the Institute: Thomas P. Conant. Dr. Louis Duncan, Prof. Wm. E. Geyer, A. E. Kennelly, Geo. B. Prescott, Jr., E. Wilbur Rice, Jr, Prof. E. P. Roberts, Prof. Harris J. Ryan, William Stanley, Jr, Dr. Schuyler S. Wheeler and Francis B. Crocker, Chairman.

This action was taken by the Council with the object of overcoming or reducing the great confusion which now exists in regard to the standards and constants of electrical conductors.

At the first meeting of the Committee, held January 10, 1890, it was decided to confine the work for the present to the three subjects of Standard of Resistance, Temperature Co-efficient, and Safe Carrying Capacity of Copper, since these are of fundamental importance.

The subject of Matthiessen's Standard alone is so confused and involved, and the discrepancies in regard to it are so great between the best authorities, that the Committee has devoted its attention almost entirely to this subject up to the present time.

A very thorough investigation of Matthiessen's work has been made by a sub-committee, consisting of Prof. Wm. E. Geyer, George B. Prescott, Jr. and the Chairman, Francis B. Crocker, and the conclusion has been reached that the most correct and satisfactory "Matthiessen's Standard" and the one which we now

recommend for general adoption is stated as follows: A soft copper wire one metre long and one millimetre in diameter ("metre-millimetre") has an electrical resistance of .02057 *B. A.* units at 0° centigrade.

From this the resistance of a soft copper wire one foot long and one-thousandth of an inch in diameter ("mil-foot") is found to be 9.720 *B. A.* units at 0° *C.*

In order to convert these values into legal ohms we may assume one *B. A.* unit to be equal to .9889 legal ohm, and the metre-millimetre value then becomes .02034 legal ohms, and the mil-foot value becomes 9.612 legal ohms.

The resistance of copper at temperature other than 0° centigrade may be determined by using Matthiessen's formula $C_t = C_0 (1 - .00387\, t + .000009009\, t^2)$ in which C_t is the conductivity at the given temperature, C_0 is the conductivity at 0° and t is the given temperature in degrees centigrade. It should be carefully noted, however, that this formula refers to *conductivity*. Therefore in order to apply it to resistance it is necessary to take the reciprocal and this should not be done by merely changing signs, which is not mathematically correct although usually given in that way. The correct modification of Matthiessen's formula when referred to resistance is

$$R_t = R_0 (1 + .00387\, t + .0000597\, t^2)$$

The sub-committee after careful consideration came to the conclusion that Matthiessen's "mile-standard" (one statute mile of copper wire $\frac{1}{7}$ inch in diameter has a resistance of 13.59 *B. A.* units at 15.5° *C*) is not the correct one, although very commonly used. Matthiessen himself did not place much confidence in this "mile-standard." The Committee, acting under instructions from a meeting of the Institute, held September, 16, 1890, has based all standards and values in this report upon soft or annealed copper, since its properties are reasonably constant and reliable. Whereas the Committee has purposely excluded from its recommendations all standards and values based upon hard copper although several were given by Matthiessen, because the hardness of copper is merely relative and the resistance of hard copper may vary between wide and uncertain limits depending upon the *degree* of hardness.

As to the fact often brought up that copper may be found which tests one or two per cent. higher conductivity or less resistance than Matthiessen's standard, we are of the opinion that

this is no real objection provided the value of the standard is definite and generally accepted. A standard which is not the highest attainable value may even be considered an advantage, since the average commercial wires will approximate to it more closely.

Although we believe the standard we recommend will answer the purpose temporarily and probably permanently, nevertheless we think that if a thoroughly correct and complete redetermination of the standard resistance of copper could be accomplished, it would be a benefit to electrical science and industry. Favorable offers in this direction have already been received by this Committee from Johns Hopkins University, Cornell University and Columbia College, and it is likely that this redetermination may be undertaken.

The following statement of the most important and reliable figures and facts given by Matthiessen will serve to show the derivation of the standard which we recommend.

A hard-drawn copper wire 1 metre long weighing 1 gramme ("metre-gramme") has a resistance of .1469 *B. A.* units at the temperature of 0° centigrade.[1]

Matthiessen also gives the resistance of a hard-drawn copper wire 1 metre long and 1 millimetre in diameter ("metre-millimetre") as .02104 *B. A.* units at 0° *C.*[1]

This implies a specific gravity of 8.89 for the copper used by Matthiessen, but unfortunately he neglected to actually determine the specific gravity.

Matthiessen's figures for relative conducting power are:[2]

Silver	100
Hard or unannealed copper	99.95
Soft or annealed copper	102.21

From these the resistance of Matthiessen's hard copper is found to be 1.0226 times that of soft copper, therefore the resistance of a soft copper wire 1 metre long and 1 millimetre diameter ("metre-millimetre") is .02057 *B. A.* units at 0° *C.*, which is the standard we recommend.

From this the resistance of 1 cubic centimetre of soft copper is found to be .000001616 *B. A.* units at 0° *C.*

And the resistance of soft copper wire 1 foot long and .001 inch in diameter ("mil-foot") is 9.720 *B. A.* units at 0° *C.*

1 Philosophical Magazine, May 1865.
2 Philosophical Transactions, 1864.

Taking one *B. A.* unit as .9889 legal ohm any of the above values may be converted into legal ohms To find the conductivity of copper at temperatures other than 0 ° Cent. Matthiessen's formula may be used, viz:

$$C_t = C_0 (1 - .00387\ t + .000009009\ t^2) \text{ or}$$
$$R_t = R_0 (1 + 00387\ t + .00000597\ t^2)$$

TABLE.

Standard Resistance at 0 ° C. of	B. A. U.	Legal Ohms.
"Metre-Millimetre" Soft Copper.	.02057	.02084
"Cubic Centimetre" " "	.000001616	.000001598
"Mil-Foot" " "	9.720	9.612

One mil-foot of soft copper at 10.22° *C* or 50.4° *F* has

Standard resistance of exactly	10	legal ohms.
Same at 15.5° *C* or 59.9° *F*.	10.20	" "
" " 23.9° *C* or 75° *F*	10.53	" "

F. B. CROCKER,
Chairman.

Discussion.

Mr. Crocker:—This report is substantially what was presented before, except that we now exclude all reference to hard copper and confine the recommendations to soft copper; this is to avoid confusion and to avoid reference to hard copper, the resistance of which is a variable and indefinite matter. In other respects I think this report is exactly in accordance with the instructions of the September meeting.

The President:—You have heard the report, gentlemen. What will you do with it?

Mr. Jo. Stanford Brown:—I move that the report be adopted with thanks.

Dr. M. I. Pupin:—I second the motion.

Mr. R. W. Pope:—Would it be well to adopt it now as the official action of the Institute?

Mr. Mailloux:—I would like to amend by adding the words; "adopted and printed in the Transactions."

Mr. Brown:—I accept that amendment.

THE PRESIDENT:—The motion is that the report be adopted and printed in the Transactions. Are there any remarks on the motion?

MR. R. W. POPE:—I rise to say, Mr. President, that the older societies are very conservative in regard to adopting anything of this kind and putting it forth as the official act of the body. They accept the reports and print them and let them go out, but they do not officially adopt them. I do not know whether there is any reason why they should not. I merely state that such is the practice. The same point came up at the recent Richmond meeting of the Mechanical Engineers in regard to a report on the tests of steam pumps, which was said to be a most excellent piece of committee work.

MR. BROWN:—I think that would be a wise thing to accept it in that shape—that is accept it though not adopt it as a society, yet have it printed in the minutes.

THE PRESIDENT:—I understand then that you make your motion to receive the report instead of adopting it?

MR BROWN:—Yes, sir, to receive it with thanks and have it printed in the Transactions.

MR. CROCKER:—I do not see that any useful end is to be subserved by simply shelving the report in this way. I do not think it is encouragement to future work, in this or any other direction. Of course the society ought to take action which is proper, and conservative, and to its best interests, but I should like to ask what will finally be the treatment of this report?

MR. E. T. BIRDSALL:—I think in matters of this kind it would be best to accept the report, cause it to be printed in pamphlet form and distributed among the members. Then, at the end of a certain time, say two or three months, the matter could be brought up again at a meeting and finally adopted and officially printed in the Transactions.

MR. GEORGE B. PRESCOTT, JR.:—It seems to me that this is a very simple question and one that the Institute could act on to-night. For a great number of years Matthiessen's standard, for the specific resistance of many metals including copper, has been universally accepted by electricians, and the actual tests that he made are duly recorded; but owing to the changes in value of various units that have been made from time to time, the text-books in recalculating these standards have adopted different values. The result is that although the electrical profession seem to be entirely satisfied to take Matthiessen's figures as a sufficiently accurate value for the resistance of pure copper, when they come to refer to the text-books for this figure, they find that different values are given. For that reason Matthiessen's standard has become meaningless, as there are a great many values for it current. Now the Institute recognized that it was very desirable that when this standard was referred to, a particular

value should be understood. This Committee was appointed simply to determine, not the absolute resistance of chemically pure copper, but simply Matthiessen's value for it, and so avoid all confusion. Now this Committee have searched through Matthiessen's original papers and have ascertained what this value was that he determined. They found moreover, that even Matthiessen's work was not entirely satisfactory and that there is considerable question as to the accuracy of this value; but at the same time they have ascertained without question what the value is, and that they have given in this report. Now it certainly will save a great deal of trouble to the electrical profession generally to have some definite standard, and it seems to me that the Institute, if it approves of the Committee's labors can accept this value as being Matthiessen's standard for the specific resistance of copper.

MR. THOMAS D. LOCKWOOD:—I fear that the gentleman who spoke last has fallen into the error of believing that because he so thoroughly understands the subject, he being, as I understand, one of the Committee, and thoroughly conversant with it from one end to the other, that every member of this Institute is in the same fortunate position. It is no doubt a natural error for him or any one else to fall into. I listened with great attention to the reading of the report of the Committee, and I am free to say that while I have not the slightest doubt that the report will be adopted, I should consider it unwise to adopt it this evening. It is of such importance that it should certainly be printed, so that every gentleman connected with the Institute should have a chance to read it and make the matter his own as thoroughly as Mr. Prescott has made it his own. After it is printed the matter can very readily be brought up again and the members can vote upon it intelligently and I have no doubt will then adopt it.

With respect to Mr. Matthiessen; for a long time his figures were accepted as being conclusive. Perhaps the gentleman himself did not believe they were so conclusive, as I believe he ultimately shot himself. It is within the bounds of probability that electricians hereafter may, (quite unreasonably of course), entertain some doubts as to the figures now presented, and for this reason we should act carefully and intelligently, and I do not think we could act intelligently upon the report read to-night, because no matter how carefully we listen, or how persistently we listen, still the ideas, as they follow one another in rapid succession, in a measure obliterate those that have come before.

MR. MAILLOUX:—I would like to make the motion that the report be adopted, as well as printed.

THE PRESIDENT:—I understand that was the original motion, and unless the Institute consent to the change that will be the motion the Chair will put. The motion, after we have commenced discussion upon it should remain as it was originally put, unless the Institute consents to the change.

MR. MAILLOUX :—That being the case I would state that I am in favor of the motion being put and passed as it was originally worded. The discussion reminds me somewhat of some of those works which are written on electrical engineering, and which though they embody the very best ability of their authors, are nevertheless by a kind of modesty on their part designated as elementary treatises only. On the same principle we have appointed a Committee, which, I think I can say contains the best material for the purpose to be found in this Institute. I have not had the pleasure of attending the meetings for some months, in fact almost a year, and I did not know much about this work or who constituted the Committee. When I heard the Chairman of the Committee read the list of members composing the Committee, I for one, felt that I would be perfectly willing to accept their views upon this matter and adopt any standard which they might give us. It is something more than an elementary treatise. Of course we all understand that the standard can only be a tempora y thing at best In 1881, the Electrical Congress at Paris adopted a so-called legal resistance unit. In other words they defined the value—the length which a column of mercury of one square millimetre of section must have in order to constitute a legal ohm. Well, we have since found that this length is not right, and we have to-day a Continental value of the legal ohm, and a British value of the legal ohm which differ from each other by a small fraction; and later redeterminations of the ohm show that neither of them is correct, and that we must increase the length of the mercury column. Now I regard this Committee's work as merely a means of effecting a temporary compromise so that the practitioners may know what to adopt pending further investigation which will be made under the auspices of that Committee, or under the auspices of some other committee appointed by some other scientific body. There is no reason to doubt that before long we shall get at the bottom of facts relating to the specific resistance of copper, yet if meanwhile we can find something which enables us to get along and make our calculations with a reasonable degree of accuracy and consistency for all practical purposes, I think we ought to be satisfied. For that reason I think we ought not to hesitate to adopt a report which gives us what is probably the nearest to the truth as the facts now seem to warrant. When it becomes necessary to amend our table, I think that it will be done in a proper manner Therefore, I hope the report of the Committee will be adopted.

MR. BIRDSALL:—The last two or three sentences that Mr. Prescott uttered, convey just the idea of my former remarks. He said that if the remainder of the Institute were satisfied as well as he was with the report, then it would be adopted. The rest of the Institute, as Mr. Lockwood very aptly put it, are not as well

satisfied as Mr. Prescott, because they are not as well versed in the subject. Therefore I would ask Mr. Brown if he would accept my first remarks as an amendment to his motion.

Mr. Lockwood:—I beg one moment's indulgence, Mr. Chairman, I do not like to speak twice upon the same subject. Somehow it seems to be implied that any such action as I advocate this evening would be disrespectful to the Committee. I beg to say that I yield to none in my respect for the Committee. I regard it, also, as being constituted of the best material we have. But if the report of the Committee is to be at once adopted, then I have to say that when the Committee was appointed it might as well have been decided to give them *carte blanche* and accept whatever report they chose to make. That is all.

Mr. Crocker:—If it is distinctly understood that this report is to be printed and submitted to the Institute, and in the course of a reasonable time the report is finally adopted, why then I offer no objections. I think the comments of the five hundred intelligent members of the Institute on a matter of this sort are very desirable.

Mr. R. W. Pope:—Mr. Chairman, that was my idea in making the remark I did—that the report should be accepted and of course printed, and theoretically it would be about the same as if we adopted it to-night, excepting this—that it would not have the official endorsement. We would then have the advantage of placing it before the public, and if any suggestions or amendments were made, of profiting by them in the final adoption of the report of the Committee, which date might be fixed at any time in the course of two or three months. It might be fixed for the May meeting when there will probably be more present from out of town than there are now. I see no reason why one or two months would not be ample time in order to elicit all the criticisms there might be upon it.

The President:—The Chairman understands the motion to be that we receive and adopt the report and that it be printed. Is this the understanding of the mover?

Mr. J. S. Brown:—My understanding is that we were to accept this report and place it on the minutes and leave it then to the consideration of the members. At least that is my desire personally. In other words that we accept it, but do not adopt it as going forth as the work of the Institute as a whole. I think the suggestion of the Secretary to leave its final adoption by the Institute until the May meeting would be wisest and best. I would, therefore, move that the report be accepted and printed in our Transactions with that understanding.

Mr. Mailloux:—If this gentleman decides to change the wording of his original motion, I think that my amendment is in order. My amendment was that it be adopted.

The President:—The resolution will have to stand as it was offered unless the Institute make no objection to the change. If

there is any objection to changing the resolution of course it would have to be put as it was offered. Amendments are in order. Of course we must receive the report before we can adopt it, and the Chair has therefore understood that the motion was to receive and adopt the report. Now if there is any objection to changing that so as to put it that we simply receive the report, the chair will have to put the motion as first understood—that we receive and adopt the report.

MR. R. W. POPE:—I move an amendment, that it be received without the word adopt. Is that in order?

THE PRESIDENT:—That is in order.

MR. POPE:—That it be received and printed in the Transactions.

(The amendment was seconded.)

THE PRESIDENT:—The amendment is that the word adopt be omitted from the motion. Those in favor of the amendment will please make it manifest by saying Aye; opposed No.

(The amendment was carried.)

MR. MAILLOUX:—I move the report be adopted.

THE PRESIDENT:—The motion before the house is that the report be received. We have just voted that the word adopt be omitted from the motion. Now the original motion comes before the house that the report be received and printed for distribution. Are there any further remarks on the motion? If not, the chair will put the motion. All those in favor of receiving and printing the report will please make it manifest by saying Aye; contrary minds No.

(The motion was carried.)

MR. MAILLOUX:—I move that the report be adopted.

(The motion was seconded.)

THE PRESIDENT:—It is moved and seconded now that the report be adopted. Are there any remarks upon this motion?

MR. BIRDSALL:—I understand the motion to mean that the report be adopted to-night.

THE PRES DENT:—That is the motion as I understand it.

MR. BIRDSALL:—I would like to offer an amendment that the report be brought up for adoption two months from to-night.

THE PRESIDENT:—The chair would hardly consider such a motion as that in order. The motion now is that the report be adopted.

MR. THORBURN REID:—I move we lay this motion on the table until the second meeting following this.

THE PRESIDENT:—It is moved and seconded that the motion lie on the table for two months. The question of time can be discussed. All those in favor of the motion that it lie on the table for two months please make it manifest by saying Aye; contrary minds No.

(The motion was carried.)

MR. R. W. POPE:—There is another committee, on the unit of self-induction.

MR. CROCKER :—The Committee on the value of the unit of self-induction, the Henry, has held one or two meetings, but is hardly yet able to report. We find that the ordinary practical values of self-induction vary from about 1/100 to 10 quadrants, therefore, the average value is a little less than one quadrant. This means that in order to express self-induction in quadrants within one per cent. you have got to go to the second or third decimal place. Therefore, in order to express all common values in whole numbers, which is desirable, it will be necessary to fix the Henry at about one-thousandth of the quadrant. Then we should very seldom have to use decimal fractions in expressing the value of self-induction. I merely state these points in order that the Institute may be informed as to the progress of our work and the general facts of the case. I think the matter better lay business over until the next meeting when we expect to be able to report.

THE PRESIDENT :—The next business will be the reading of a paper by Dr. Louis Bell upon "The Theory of Compound Winding for Constant Potential."

A paper read at the 51st Meeting of the American Institute of Electrical Engineers, New York, November 18th, 1890, President W. A. Anthony in the chair.

ON THE THEORY OF COMPOUND WINDING FOR CONSTANT POTENTIAL.

BY DR. LOUIS BELL.

I wish to preface this paper by stating that there is in it nothing revolutionary, nor any radically novel attempt to discuss a problem that has already been treated by more skilful hands than mine. It is simply an effort to bring the theory of compound winding to a practical basis, indulging in no complicated formulæ and introducing no constants that can not be conveniently determined in practice. To this end I shall consider the subject synthetically rather than analytically, starting from the plain shunt winding and considering those terms which must be introduced to take account of the failure of shunt winding to give constant potential. I do not think that compound winding should be considered as anything more than a method of improving a shunt machine already well designed; it certainly never should be called upon to avoid the evil effects of bad design.

For the systematic treatment of the subject I would introduce the following notation, which differs slightly from that most often employed, but is convenient for the purpose in hand.

Let C_x represent the current on the line;

C_s, the current in the shunt coils;

R, the resistance of shunt coils;

r, the resistance of armature and series coils combined;

E, the E. M. F. of the machine running light;

e, the fall of potential due to armature resistance;
N, the number of convolutions in shunt coils;
n, the number of convolutions in series coils;
m, the number of convolutions on armature;
a, b, c, the three terms of n;
l, the angle of lead;
q, the magnetic resistance of machine on open circuit;
q_x, the magnetic resistance of machine at full load, and
p, the value of q_x divided by q.

I shall consider especially the long shunt arrangement, though, as will be seen later, the short shunt leads to practically the same results.

The first and obvious cause of fall of potential at the terminals of the machine is due to the fact that the armature has a resistance; as the current increases, a certain amount of potential is absorbed in the armature, and consequently the available E. M. F. at the terminals falls. The amount of decrease is evidently proportional to the resistance in the armature and the current through it.

$$e = r\ C_x$$

To compensate this loss we must furnish an increase of induction sufficient to produce e additional E. M. F. If the field magnets and the armature core were quite unsaturated, that is, if we could neglect to change in permeability, we should simply increase the total ampere-turns to an amount sufficient to compensate the fall in potential. Let a be the series turns capable of producing this at full load, neglecting change in permeability, then

$$a = \frac{N r}{R}$$

But the permeability decreases as we put on this additional induction. The magnetic resistance q at the start will have increased to a certain other value q_x. We must therefore increase the number of series turns to take account of this added resistance, and we have

$$a = \frac{N r}{R} p$$

It will be observed that this quantity does not have to be multiplied by the coefficient of leakage because we have simply reduced the shunt turns to equivalent series turns, and conse-

quently have already included it. This formula will be at once recognized as practically equivalent to that given by Thompson in his well-known book, and at this point Thompson drops the investigation, merely mentioning incidentally some of the other causes which necessitate compounding. Thompson's formula:

$$\frac{Z}{S} = \frac{r_s}{r_s + r_m} \times \frac{q_1}{q_0}$$

is not quite the same as mine for the reason that while his q_0 is proportional to the permeability when there is no external current, his q_1 is proportional not to the permeability at maximum current, but to the mean permeability between no load and full load. Hence if Z is taken as the number of shunt turns required to give the desired potential on open circuit, Thompson's formula will inevitably give too few series turns for the reason just mentioned, if for no other. But since by adding series turns the magnetic resistance is increased, the shunt winding which produces E volts on open circuit will not do so at full load. Additional series turns must then be employed to compensate for this and since the change in shunt ampere turns must be from NC to NCp, the number of series turns to be added to compensate for the decreased effect of the shunt will be

$$b = \frac{\frac{NE}{R}(p-1)}{C_x}$$

This term also includes the coefficient of leakage for the same reason as before.

It will be noticed that both a and b include the quantity p, and here is met the principal difficulty in effecting the compounding. It is my purpose to show that this quantity p can be obtained from known data with sufficient accuracy for the purpose in hand. It will evidently be a quantity little in excess of unity and in practical machines may range from 1.05 to 1.50, although in certain cases it may even exceed the latter figure. Thompson gives the case of a certain Siemens compound machine in which p would be over three. This, however, must have had a very insufficient amount of iron in the magnets or the armature core, or both. We fortunately do not have to deal with dynamos of such bad design very frequently. Provided we have known enough about the iron to be used in a given machine to design the shunt winding intelligently, we shall also

know enough to design the compound winding. If we have at hand the characteristic curve of a similar shunt machine, p can be determined very easily from it. I give herewith the data for the Manchester machine described by Dr. Hopkinson. I have simply taken the apparent permeability from his characteristic curve.

When magnetizing force equals		induction equals	
		magnetizing force	
1,000		470	
2,000		465	
3,000		460	
4,000		460	
5,000		460	
6,000		451	
7,000		443	
8,000		425	
9,000		400	
10,000		377	
11,000		353	
12,000		333	

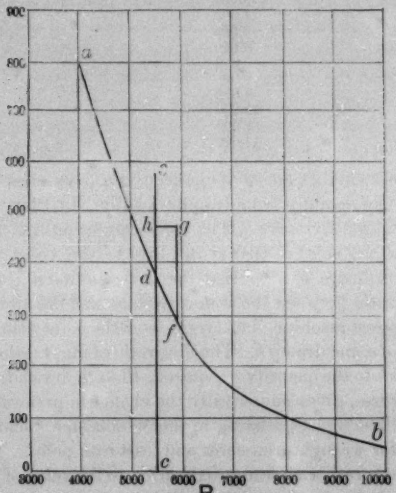
If the permeability curve of the iron is known even approximately we can obtain p from it quite readily. In Plate 1, let $a\,b$ be the permeability curve. The permeability at no load corresponds to the point d, that at full load to the point f. Now lay off the ordinate $d\,e$ so that the ratio between $d\,e$ and $c\,d$ equals the ratio between the iron resistance and the air resistance of the proposed machine, then lay off from the point f the ordinate $f\,g$ equal to $d\,e$, and draw $g\,h$. Then the ratio of the lengths $h\,c$ and $e\,c$ corresponds to the quantity p required. Plate 2, drawn from Hopkinson's figures, gives numerically the change in permeability for cast-iron. The figures relating to the Manchester machine were, of course, for wrought-iron cores and cast-iron poles. It is thus evident that a rather close approximation to the value of p can be made if we know anything about the iron that we are going to use. Now as we shall see later, and as is indeed evident at the start, a and b are of about the same order of magnitude. I do not think that b will ever be negligible in a practical machine unless a is also negligible, and there is, therefore, little need for compounding. When the magnets are anywhere near saturation b will be much larger than a, but both a and b combined will not give the proper number of series turns for constant potential unless the armature is run with no load, and therefore produces no demagnetizing effect. The approximate demagnetizing effect of the armature has been investigated by Hopkinson and is as follows:

$$4\pi C\frac{m2l}{2\pi}=4lmC.$$

In other words, it is due to the convolutions included in twice the angle of lead. For constant potential we must add then c turns in series equal

$$\nu m\frac{l}{\pi}$$

It is evident at once that a change in lead will affect the com-



B
Plate I.

pounding; an increased lead will cause the machine to under-compound, a decreased lead to over-compound. As, however, the amount of distortion in the field depends largely on the shape of the extremities of the pole-pieces, the above uncertain factor in compounding can often be gotten rid of by a little care in designing the poles.

It is obvious that the term c is to include the coefficient of leakage. Uniting now, the three terms, a, b and c, we have finally, as the completed formula for compound winding, the following:

$$n = p\left(\frac{Nr}{R} + \frac{\frac{NE}{R}(p-1)}{C_x} + \nu m\frac{l}{\pi}\right)$$

It now may be interesting to see how well this formula applies to practical machines. I regret to say that I have been unable to get full details of any machine which gave exact compounding, but I have obtained an assortment sufficiently large to show in a general way the application of the formula.

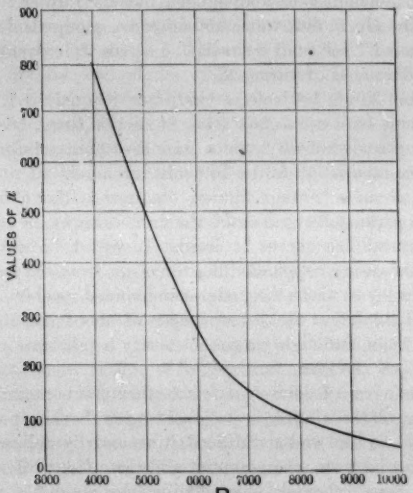


Plate 2.

Machine No. 1, 110 volts, 8 amperes, over-compounded 2 volts. For this small machine take p equal to 1.2. Applying the formula, a equals 14, b equals 44, c equals 15; total series turns by formula, 73; actual number, 90.

Machine No. 2, 110 volts, 3 amperes, over-compounded 3 to 5 volts; a equals 2, b equals 7, c equals 7; total series turns by formula, 16; actual number, 28.

Machine No. 3, 500 volts, 20 amperes; p equal to 1.1, a equals 3, b equals 15, c equals 14; total turns by formula, 33, actual number, 60. Machine over-compounded 3 to 5 volts.

Machine No. 4, 110 volts, 200 amperes, over-compounded 5 volts; p equals 1.05; lead rather large; a equals 2, b equals 2, c equals 5; total turns required, 9; actual number, 12.

Machine No. 5, 500 volts, 60 amperes; p equals 1.05, over-compounded about 7 per cent. a equals 3, b equals 5, c equals 4; total 12; actual number, 32,

Machine No. 6, 500 volts, 90 amperes; p equals 1.05; a equals 3, b equals 4, c equals 4; total turns should be 11; actual number, 19. Machine over-compounded like the former.

Machine No. 7, 500 volts, 150 amperes, p equals 1.05; over-compounded 7 per cent. a equals 3, b equals 3, c equals 3; total, 9; actual number of turns, 28.

Machine No. 8, 100 volts, 30 amperes, p equals 1.1, a equals 13, b equals 46, c equals 25; total, 84; actual turns, 56. Machine over-compounded about 2 volts, and the shunt winding did not bring the potential up to the 100 volts intended.

These are merely rough figures, obtained without the use of even the permeability curve of the iron—simply by estimation from permeability curves at hand. It will be seen, however, that all the over-compounded machines got from the formula too few series turns, while the under-compounded machine gave too many. I think that the discrepancy is of about the right order of magnitude, although we cannot verify it accurately without the curve of the iron.

It remains to consider some few further points regarding compounding. If the winding is designed to give the same potential at practically no load and at full load, it necessarily follows that at some point between, the potential will rise; for, while the magnetizing force changes along a line, the magnetic resistance changes along the corresponding curve between the same points.

An approximate idea of the amount of irregularity in potential may be gained from Fig. 1. Connect in this diagram d and f by a straight line. From its middle point drop an ordinate upon the axis. The length intercepted between the straight line and the permeability curve then gives the error in compounding, by comparison with the total change of magnetic resistance.

It should be noted that the formula I have given determines the compound winding for a given assumed potential produced by a shunt coil alone; therefore, if the machine is to be run at a slightly different speed from that at first intended to produce this given voltage, the compounding is still correct, for it per-

tains to the voltage, rather than to the speed. If the speed of a correctly compounded machine be raised and the current in the shunt kept constant, it will tend to over-compound. If the current in the shunt is allowed to increase, the change in resistance will decrease the relative power of the series coils, and the machine will under-compound.

It now only remains to glance at some of the formulæ heretofore given. That of Thompson I have already incidentally discussed. That of Frölich (perhaps the best we have had) should receive mention here. Frölich takes account of the change in permeability by introducing a quantity f. Like Thompson, his formulæ involve two points of saturation. It should be said here that there is no special black magic about the half saturation point of a dynamo, any more than about the three-sevenths or the seven-thirteenths saturation point. In very many cases it happens to be about the point at which the machine is used. Kapp, in his well-known and excellent book, takes account of the change in permeability with a fair degree of approximation, by the use of his tangent function.

All these attempts at the proper theory of compounding depend, like the one which I have just presented, on forming some sort of an estimate on the change in magnetic resistance of a dynamo, due to adding a certain amount of induction. Without some approximate method of finding this quantity, the problem is indeterminate, and the special thing which I have endeavored to show is that from the data already at hand about iron, we can form a sufficiently close estimate of the value of the coefficient p, without either knowing the characteristic of the machine, or indulging in endless approximations. I hope that some practical designer will look into the values that p takes in his machines, and I believe that it can be readily enough obtained to permit of quite accurate compounding, by the use of the formula I have given. The introduction of the terms b and c I regard as important. Their existence has been well known, but I do not think they have received the attention that I have endeavored to to show they deserve.

[Note Communicated by Dr. Bell since the Discussion.]

I am much indebted to Mr. J. B. Entz for calling attention, in the course of the discussion to an apparent increase in the mag-

netic resistance of the air gap as the load is increased. I say apparent increase, because it is hard to separate increase of magnetic resistance from counter magnetomotive force supposed by some to be caused even while the armature is running without load. In applying a formula similar to my own, and, like it, including change in permeability and armature reaction as well as armature resistance, Mr. Entz found that his formula gave only about 80 per cent. of the series turns actually required. This was in the case of Edison machines, and while not knowing the exact character of his formula I am unable to compare it fully with my own, I am inclined to agree with him that the observed discrepancy is probably due to increase of the resistance of the air space rather than to counter magnetomotive force. It has been before this pointed out, that while a dynamo is running, the lines of force do not pass radially across the air space, but are skewed by the magnetism of the armature so that the actual path must be somewhat greater than the apparent one. I am surprised, however, that the difference should reach the amount observed. It certainly would not do so in machines like the Wenstrom, having a very small air space. The total effect on the magnetic resistance of the machine would be of course proportional to the ratio between the magnetic resistance of the iron and of the air gaps, and would increase when the relative value of the latter increased. The amount of increase of air resistance due to increased load probably varies quite systematically with the ratio of armature ampere turns to field ampere turns, but the exact amount of this variation, disturbed as it is by the effect of the polar extremities, is a subject for further investigation. If the lines of force were skewed 45 or even 30 degrees from a radial direction over any considerable portion of the air gap there would certainly be a very perceptible effect on the magnetic resistance of the dynamo. In any case, however, if the amount of skewing can be even roughly estimated this added magnetic resistance can very readily be taken account of in the formula I have proposed. To do this it would only be necessary to take account of the change in laying out the line $f\ g$ in Plate 1. On drawing $g\ h$ we should then have a value of p, including the change in resistance due to the cause mentioned. I shall take up this question in detail, for a sufficiently simple and convenient method of estimation can probably be found that will enable the correction to be introduced into my formula by the above very easy method so as to give a result sufficiently exact for all practical purposes.—L. B.

Discussion.

Mr. Townsend Wolcott :—In the paper by Mr. Esson, which was published in the *Electrical World*, there is a formula given for compounding machines, or rather a variation of the ordinary formula for calculating the field turns. Mr. Esson adds to the formula of Hopkinson another term consisting of thiry-three per cent. of the ampere turns on the armature. There is one other term however in a compound machine. This thirty-three per cent. of the ampere turns does not take into consideration the loss of potential due to resistance of the armature. Mr. Esson says that in his Phœnix dynamo he adds 165/1000 of the number of armature turns to each field coil. That is to compensate for the resistance in the armature, and backward induction has rather more effect in reducing the potential of the terminals than the armature resistance has, and he stated that in the Victoria dynamo it is about twice as great. In connection with that backward induction I would like to make another remark. According to the figures of Mr. Esson and some others, the backward induction from a Gramme armature is greater than from a Siemens, which is contrary to what we would expect. By ampere turns on the armature he means one half the product of the convolutions by the current. Now in a Gramme armature, each turn surrounds only half the iron in the armature, while in the Siemens it surrounds the whole iron. But as a practical fact Mr. Esson thinks that thirty-three per cent. for the Gramme armature is about right. I would like Dr. Bell's opinion on that subject, as I see he does not use a term of that kind.

Dr. Bell :—When it comes to the question of armature resistance and the relative effect of armature resistance and the armature turns, it depends entirely upon the type of machine, as I showed by the figures there. The attempts that have generally been made to compound have consisted in a sort of guessing at what was about the right proportion and finding it experimentally for one type of machine. Once found, it holds fairly well for that type. Now my term c takes care of the armature turns and the term a takes care of its resistance. I think the reason why Mr. Esson found the Gramme armature gave more distortion to the field was due to the fact that the Gramme armature admits of forming definite poles, so to speak more readily. It is not in this case cross-magnetizing a cylinder of iron of considerably greater length than diameter, but it is magnetizing the ring so as to get one pole at each brush, which I think gives a rather more definite pole at that point and makes more trouble in the field. Now its effect for the same number of ampere turns in producing backward induction may be the same. As a matter of fact I think the Gramme armature produces a greater disturbance of the field and therefore must run with more lead. Therefore the term b would be larger.

MR. WOLCOTT:—It would seem to me that the two things were nearly synonymous—the backward induction and the distortion of the field. In consequence of the tendency of the armature to form poles cross-wise, there results a distortion of the field. That is what we call the backward or cross-induction, as I understand it, and as the intensity of induction is always less in the Siemens armature, it would seem that the variations of the amperes would naturally cause greater variations in that part of the magnetism of the armature which is due to the current in the armature wire. The Gramme armature has usually double the induction of the Siemens, unless the latter is made with the centre cut out.

DR. BELL:—The backward induction and the distortion are two totally different things. The presence of the distortion is the unfortunate cause of the backward induction. Of course they are really due to the same ampere turns; but if there is anything that causes a violent local distortion of the field it will raise such trouble about sparking that we shall have to set the brushes over, and in doing that, shall have the plane of magnetization of the armature so that it produces a severe backward induction. If we so arrange the pole-pieces that there will not be any sparking to speak of, you can run any kind of an armature you please and run it in either direction and it will not spark, and you can set the brushes without lead. If you arrange the pole-pieces so that you can get a certain amount of distortion without causing sparking you need not trouble yourself very much about the backward induction.

MR. A. E. KENNELLY:—I wish to point out one thing, namely that it is good practice to over-compound dynamos—for this reason: that you seldom, if ever, have your delivery at the machine terminals. If your attention be confined to the point of delivery, say a mile away from the dynamo, it may be interesting to know that under all loads that dynamo is delivering a constant potential difference at its terminals, but if you have to maintain a constant potential difference at your feet, the fact that the potential difference is constant at the machine terminals does not altogether help you. Therefore if you over-compound your machine so as to take care of the fall of potential in your conducting wire, which is in a certain sense a resistance of the armature according to the extended view that the author of the paper to-night gives, by including a series winding with the armature, you then have the means of maintaining the potential difference constant at your point of delivery. If the resistance of the leads to your supply is not definite or constant, then you cannot so compound the machine as to give a constant delivery of potential difference, because one of the factors is a variable. For that reason the only source of adjustment left you is a shunt, for example, upon the series winding. That is an arrangement which is comparatively familiar. And while the paper gives us very clearly and ex-

plicity three of the prominent factors that are necessary to take into account for the compounding of machines, I think that the fourth factor deserves some attention. Also the third term c is really only an approximation—c is more complicated than that looks unfortunately; although, perhaps the complication is not of very great practical importance.

DR. BELL:—Mr. Kennelly is quite right in saying that it is desirable to over-compound a machine, and of course by including the resistance of the line in that small r you have got the compounding for a distant point as has been pointed out by Thompson. We need some slight further change in the formula, however, because you still further increase the saturation, and therefore the value of p would rise slightly; but if you know about how much you want to over-compound the machine, you can apply the formula just as readily to over-compound for a given rise of potential as for straight compounding. It simply requires using a different value of your induction. The armature term is unfortunately more complicated; but the term c is a fair approximation to it in ordinary cases. The cross-induction in the armature, particularly where the amount is considerable, assumes a very disagreeably complex form, but that is perhaps as ready an approximation as the present state of the art admits, and I have omitted together with the further complications of the armature, a considerable number of small periodic terms, etc., which are of the second order compared with those three more prominent ones.

MR. CROCKER:—I agree with Mr. Kennelly that the formula for practical purposes, ought to consider the line of resistance just exactly as much as the armature resistance, and there always is some line resistance with every armature resistance in practical cases. A maker should either have his machines compounded with reference to the use they are to be applied to or adopt an approximate over-compounding which would apply to most cases. We know that most isolated plants ought not to have over five per cent. drop between the dynamo and lamps and they rarely have less than three per cent. drop; therefore four per cent. over-compounding would probably be within one per cent. of right in most practical cases. I would also like to say, in regard to the third term c of Dr. Bell's formula that it may have considerable value without any distortion at all. The brushes can stand practically at right angles to the line joining the centres of the pole-pieces and yet that "back-induction" as it is called will still have a very considerable effect in reducing the E. M. F. I have some ideas on that subject and I am now proving them experimentally. As to "back-induction" I think that is a very unfortunate name. I consider "back-magnetization" better. The word induction is over burdened, I think. It is used in "static-induction," "self-induction" and "mutual induction" as well as in its ordinary sense.

MR. JUSTUS B. ENTZ:—I have used that formula and compared it with the averages of about three thousand machines of the Edison Company. These averages were revised about every six months and after a while there might be a difference of about one quarter per cent. between them; and in no case did that formula give more than 80 per cent. of the additional ampere turns necessary for compounding. I got an exact measurement of the angular movement brushes on the machine. I knew the point on the curve of the dynamo as we call it. That is to say the percentage of increased magneto motive force necessary for one per cent. increase of lines of force. Say the ratio of the increase of ampere turns to the increase of magnetism, and most of them were at such a point on their characteristic curve that it requires about two per cent. additional ampere turns of the field to raise the voltage of the machine 1%. If we had five per cent. drop in the machine at the point 2 to 1 on the curve, it would need for that ten per cent. more armature turns. The term c is taken nearly the same way it is there, and is added to the armature term a. But there is an additional leakage from the pole-pieces due to the back ampere turn in the armature which is also calculated from knowing the point on the curve. But in no case did the sum of these terms come over 80 per cent.

DR. BELL:—May I ask how you got the value of p?

MR. ENTZ:—I say I took the term a and b as one. I found by actual test on the machine just what per centage of ampere turns was necessary for a given increase in voltage or for a given increase in induction.

DR. BELL:—I do not quite understand whether you got that term p at all. If you did not take the term p corresponding to the real magnetic resistance, of course you would not get a correct result.

MR. ENTZ:—It was taken. You have got a simple ratio of magnetic resistance between no load and full load. Say the resistance of the total circuit has increased at the rate of two per cent. for one per cent. increase of induction in the machine, that would correspond to a point of 3 to 1 on the curve, and the ampere turns would have to be increased two per cent. for the increased resistance of the magnetic circuit plus one per cent. for the increased lines.

DR. BELL:—That makes it a linear increase, which it certainly is not.

MR. ENTZ:—Of course it varies. It is a curve like any saturation curve, and is different for different voltages of the same machine. For instance if the iron is at such a point that for one per cent. increase of induction its resistance increases two per cent., then in calculating the additional ampere turns necessary, they would be directly proportional to the drop in volts for the air magnetic circuit, and they would be; not directly proportional for the iron circuit but proportional, to the increased induction times the increased resistance of the iron.

DR. BELL:—I think I see the difficulty now. If you compound the air resistance and the iron resistance, you will get the apparent permeability of the machine, as you seem to have done in that case. You could have any given value as saturation, a certain definite value of the ampere turns, but that is not at all getting to the point p in the formula.

MR. ENTZ:—I know. You have got a quantity there for p of about 1.1. If the drop in that machine were five per cent. it would mean that the magnetic resistance of that machine had increased ten per cent. what I would call three; 1 on the curve. It comes to exactly the same thing.

DR. BELL:—But how do you account for the extra ampere turns required?

MR. ENTZ—I do not think the lines of force stand in the same plane.

DR. BELL:—In other words you think there is a specific increase of resistance due to the induction of the armature?

MR. ENTZ:—No; due to the tension of the lines.

DR. BELL:—Due rather to the cross-induction of the armature—skewing the line so to speak.

MR. ENTZ:—Yes, due to the line being dragged around after the armature, and I found a constant to express those terms which is proportional to the current intensity of the armature over the magnetic intensity of the field.

DR. BELL:—Have you tried that formula on any exactly compounded machine?

MR. ENTZ—On an exactly compounded machine.

A MEMBER:—How does Dr. Bell calculate the angle of lead? In a case I had the other day, in a 10,000 watt machine a and c were exactly equal.

DR. BELL:—You can get the angle of lead in any type of machine that you know anything about, quite approximately. For instance in a machine that I had to consider it varied from $\frac{1}{8}\pi$ to $\frac{1}{16}\pi$. Now if the cross-induction is really a large quantity with no lead at all, that would fully account for all the discrepancies we have just considered. Then you could not determine any armature turns that would account for it at all. But that does represent it approximately, I think, though it would be too small rather than too large.

MR. ENTZ:—If the brush was kept on the neutral line according to that formula the volts would only drop due to the drop of the armature. That is if the field was independently excited.

DR. BELL:—Plus possible distortion that you mentioned.

MR. ENTZ:—I say that is so according to that formula, but it is not so, because I have tested machines in that way.

DR. BELL:—That is probably due to the increase of resistance in the air space.

MR. ENTZ:—That is what I think. I do not think the lines stand in the same place as at no lead.

MR. MAILLOUX:—With regard to the role played by the armature current. I hold views similar to those just advanced by Mr. Entz; and in endeavoring to analyze the reactions occuring there, I have for some years past been accustomed to consider the change of magnetic resistance under different conditions as being due to the change of path of the magnetic lines of force. The phenomenon is indeed one which has been long since well recognized. If I am not mistaken it has been commented upon in scientific papers, notably by Mr. Snell, in England. At any rate it is so familiar to me that I would scarcely be able to tell where I first learned of it—where the impression first arose. I think if we stop to analyze the reaction which will take place in the presence of current in the armature, we will very quickly see that the presence of current and the reaction attending it must change the distribution of the lines of force. That change of distribution is followed by a rapid re-distribution at each commutation of a section. We must consider it as taking place at a very great velocity and being distributed or broken up again at a very great velocity, but not necessarily at the same velocity. Now there can be no doubt, and I think the facts substantiate it, that when there is current passing through the armature it distorts the lines of force. It is as if the armature wires moved them bodily a short distance, the space of one section, more or less, at each commutation and thus lengthened their lineal path, so that the portion of the total or gross magnetic resistance which is due to the air gap will be materially lengthened, because the lines of force are stretched out by the current, carried onward, and they will not cut the armature wires until they have been carried forward further the greater the armature current. I think that is really where we get the difference of magnetic resistance between no load and large load, although a certain variation takes place in the magnetic circuit itself in consequence of hysteresis There is no doubt a slight disturbance there which is the analogue of the spurious resistance due to self-induction in electrical circuits. We have an inductive, magnetic resistance, so to speak, a "live" magnetic resistance, truly inductive in one sense.

I have noticed further that this armature resistance increment is a certain function of the lead. That is another fact which nearly all who have had practical experience with dynamos must have noticed. I would like to know from the author of the paper or any other person present if there is any information as to what kind of a function it is of the angle of lead. I think it is a matter of the greatest interest, theoretically and practically.

DR. BELL:—I think Mr. Mailloux is right in saying that the increment of resistance is a function of the angle of lead, but I am afraid it is also a function of the shape of the pole-pieces and two or three other things of a somewhat difficult character to bring

into any simple treatment. If the increment so to speak of air resistance, plus perhaps an increased change of induction, because both may enter as well as one, rises as Mr. Entz's remarks would seem to indicate, to a perceptible amount at least in smooth armatures, I should think that a fair approximation might be made by the process, which is apparently tried practically of adding an estimated amount to the air resistance. In other words this would amount to adding a certain amount to the air resistance. I attempted to get this quantity p by taking the permeability curve of the iron and laying off an ordinate from the point of no load, a quantity proportional to the air resistance. Now taking the equal line $f\,g$ and drawing $g\,h$, I obtain this quantity p. If that added air resistance amounts to a considerable quantity I should say that it might be very readily taken care of in any type of machine about which we know anything, by simply taking a distance here on $f\,g$, say to a point g, in other words, being mindful of the fact that the air resistance increases as well as the iron resistance. If we can form any approximate idea as to the amount of this increase we can take account of it in this very simple may, which would give the value of p, that would give the correct number of turns for compounding. If the air resistance varies in any lineal way or any reasonably simple sort of curved way, that graphical method would give a value of p to take into account the increase in air resistance. I would like to have Mr. Entz try that.

MR. ENTZ:—I say we found it very nearly proportional to the current density of the armature divided by the magnetic density.

DR. BELL:—In other words it varies lineally.

MR. ENTZ:—Yes.

DR. BELL:—In that case it may be very easily taken into account by simply bearing in mind the lineal change in laying off the second line $f\,g$. That would give us the value of p including the total change in resistance, which would then give the correct number including that, and this would be a comparatively simple term added to our theory of compounding. Being a lineal quantity it is very readily handled and will admit, I think, of getting the correct value of p by this graphical method which will bring the commutation out right.

THE PRESIDENT:—Are there any further remarks on the paper of Dr. Bell? If not we will call for the second paper on the programme, by Mr. Steinmetz.

A paper read at the 51st meeting of the American Institute of Electrical Engineers, November 18th, 1890.

A NEW METHOD OF ANALYZING ARMATURE REACTIONS OF ALTERNATORS.

BY CHAS. STEINMETZ.

It results from the discussion of Mr. Reid's paper, read before the Institute last month that the assumption of the constancy of N, the field magnetism and ρ, the magnetic resistance of an alternator, can not generally be considered true enough to be accepted, and therefore the equations given by Mr. Reid can not be considered complete, although being very valuable on account of their simplicity.

To complete those equations by taking into consideration the pulsating nature of the field magnetism, which fluctuates by more than 50% in the kind of alternator tested, as shown in the paper of Messrs. Tobey and Walbridge, and by considering the periodical variations of ρ, we start from the fundamental equation (6), given in Mr. Reid's paper.

By substituting equation (8), this equation (6), takes the form:

$$E = -\frac{d}{dt}\left\{ S\,N \cos\frac{2\pi}{T}t + S^2\,\mu\,i \right\}, \qquad (1)$$

where we introduce, instead of the magnetic resistance ρ, its inverse value, the magnetic conductivity,

$$\mu = \frac{1}{\rho}, \qquad (2)$$

in order to make the equation hold also for the other method of considering the phenomena, by means of the term "coefficient of self-induction," this being proportional to the magnetic conductivity by the equation:

$$L = S^2\,\mu. \qquad (3)$$

Substituting: $E - Ri$, introducing the transformation:

$$-\frac{2\pi}{T}t, \tag{4}$$

and substituting,

$$s = \frac{T}{2\pi}, \tag{5}$$

we get:

$$s\,R\,i = -\frac{d}{d\varphi}\left\{S\,N\cos\varphi + S^2\,\mu\,i\right\}; \tag{6}$$

or, expanded:

$$S^2\,\mu\frac{di}{d\varphi} + s\,R\,i + S^2\,i\frac{d\mu}{d\varphi} - S\,N\sin\varphi - S\cos\varphi\frac{dN}{d\varphi}, \tag{7}$$

as the complete differential equation of the phenomenon.

Now we have to consider N, ρ and μ also as periodic functions of the time t, and therefore of the angle φ; but as such functions, which have two periods during one period of the alternating current, and therefore, as univalent periodics, they must have the form:

$$\begin{aligned} N = F &+ x_1\sin 2\varphi + x_2\sin 4\varphi + \ldots. \\ &+ \beta_1\cos 2\varphi + \beta_2\cos 4\varphi + \ldots. \\ \mu = \sigma &+ \zeta_1\sin 2\varphi + \zeta_2\sin 4\varphi + \ldots. \\ &+ \xi_1\cos 2\varphi + \xi_2\cos 4\varphi + \ldots. \end{aligned}$$

In the first approximation, by using only the first three terms of each expression, and making some changes, these equations assume the form:

$$\left.\begin{aligned} N &= F - G\cos(2\varphi - \alpha) \\ \mu &= \sigma - \gamma\cos(2\varphi - \vartheta) \end{aligned}\right\} \tag{8}$$

Substituting these values in equation (7), we get:

$$S^2\left\{\sigma - \gamma\cos(2\varphi - \vartheta)\right\}\frac{di}{d\varphi} + s\,Ri + 2\,S^2\gamma i\sin(2\varphi - \vartheta) -$$

$$- SF - SG\left\{\sin\varphi\cos(2\varphi - \alpha) + 2\cos\varphi\sin(2\varphi - \alpha)\right\}, \tag{9}$$

the complete differential equation of an alternator, where α is the angle of lag of the field magnetism, and ϑ the angle of magnetic lag, caused by hysteresis and eddy-currents.

This equation (9) is integrated by the periodic function:

$$i = A\sin\varphi + B\cos\varphi + C\sin 3\varphi + D\cos 3\varphi, \tag{10}$$

if we neglect the exponential factor of the integral, which gives the phenomena in starting the alternator, but disappears after a few thousandths of a second.

Value (10) substituted in equation (9) gives us, after cancelling all the terms of tertiary order: $G\gamma$, $C\gamma$, $D\gamma$, etc., and making a few changes, for the determination of the coefficients A, B, C, D, an identical equation in $\sin\varphi$, $\cos\varphi$, $\sin 3\varphi$, $\cos 3\varphi$.

The coefficients of this equation placed $= 0$, give us four equations, from which we get:

$$A = -\frac{S}{s^2 R^2 + \sigma^2 S^4}\left\{ s R F + \frac{G}{2}(\sigma S^2 \sin a - s R \cos a) - \frac{\gamma F S^2}{2}\sin\vartheta \right\}$$

$$B = -\frac{S}{s^2 R^2 + \sigma^2 S^4}\left\{ \gamma F S^2 - \frac{G}{2}(\sigma S^2 \cos a + s R \sin a) + \frac{\gamma F S^2}{2}\cos\vartheta \right\} \quad (11)$$

$$C = -\frac{3S}{2(s^2 R^2 + 9\sigma^2 S^4)}\left\{ \frac{\gamma F S^2}{\sqrt{s^2 R^2 + \sigma^2 S^4}}[sR\sin(\vartheta - \varepsilon) + 3\sigma S\cos(\vartheta - \varepsilon)] - \sigma G[s R\cos a - 3\sigma S\sin a] \right\}$$

$$D = -\frac{3S}{2(s^2 R^2 + 9\sigma^2 S^4)}\left\{ \frac{\gamma F S^2}{\sqrt{s^2 R^2 + \sigma^2 S^4}}[s R\cos(\vartheta - \varepsilon) - 3\sigma S\sin(\vartheta - \varepsilon)] + \sigma G[s R\sin a + 3\sigma S\cos a] \right\}.$$

These values substituted in equation (10), give, after cancelling again the terms of tertiary order, substituting (5), and calling the number of periods per second,

$$n = \frac{1}{T}, \quad (12)$$

the expression for the armature current in the alternator:

$$i = \frac{2\pi n F S}{R\sqrt{1 + \frac{4\pi^2 n^2 \sigma^2 S^4}{R^2}}}\sin(\varphi - \varepsilon) - \frac{\pi n G S}{R}\left\{ \frac{\sin(\varphi - a - \varepsilon)}{\sqrt{1 - \frac{4\pi^2 n^2 \sigma^2 S^4}{R^2}}} + \frac{3\sin(3\varphi - a - \eta)}{\sqrt{1 + \frac{36\pi^2 n^2 \sigma^2 S^4}{R^2}}} \right\} - \frac{2\pi^2 n^2 \gamma F S^2}{R^2\sqrt{1 + \frac{4\pi^2 n^2 \sigma^2 S^4}{R^2}}}\left\{ \frac{\cos(\varphi - \vartheta)}{\sqrt{1 + \frac{4\pi^2 n^2 \sigma^2 S^4}{R^2}}} - \frac{3\cos(3\varphi - \vartheta - \varepsilon - \eta)}{\sqrt{1 + \frac{36\pi^2 n^2 \sigma^2 S^4}{R^2}}} \right\} \quad (13)$$

where we introduced the substitutions:

$$\frac{2\pi n \sigma S^2}{R} = \tan \varepsilon , \tag{14}$$

$$\frac{6\pi n \sigma S^2}{R} = \tan \eta = 3 \tan \varepsilon, \tag{15}$$

the angle of lag of the armature current.

From this equation (13) we get the square root of the mean square, or the effective current:

$$I = \frac{\sqrt{2}\pi n F S}{R\sqrt{1+\frac{4\pi^2 n^2 \sigma^2 S^4}{R^2}}}\left\{1 - \frac{G}{2F}\cos\alpha - \frac{\pi n \gamma S^2 \sin(\vartheta - \varepsilon)}{2R\sqrt{1+\frac{4\pi^2 n^2 \sigma^2 S^4}{R^2}}}\right\}. \tag{16}$$

The first term of this expression is the value given in Mr. Reid's paper under equations (15), (17), (19), and therefrom the E. M. F.:

$$E = \frac{\sqrt{2}\pi n F S}{\sqrt{1+\frac{4\pi^2 n^2 \sigma^2 S^4}{R^2}}}\left\{1 - \frac{G}{2F}\cos\alpha - \frac{\pi n \gamma S^2 \sin(\vartheta - \varepsilon)}{2R\sqrt{1+\frac{4\pi^2 n^2 \sigma^2 S^4}{R^2}}}\right\}. \tag{17}$$

For a CONSTANT POTENTIAL alternator, where the self-induction of the armature current is low enough to be neglected, equation (17) assumes the form:

$$E = \sqrt{2}\pi n F S\left\{1 - \frac{G}{2F}\cos\alpha - \frac{\pi n \gamma S^2 \sin(\vartheta - \varepsilon)}{2R}\right\} \tag{18}$$

Here again the first term is given in Mr. Reid's paper under figure (20).

For a CONSTANT CURRENT alternator, where the self-induction of the armature is so high that the other terms may be neglected, equation (16) takes the form:

$$I = \frac{F}{\sqrt{2}\sigma S}\left\{1 - \frac{G}{2F}\cos\alpha - \frac{\gamma}{4\sigma}\sin(\vartheta - \varepsilon)\right\} \tag{19}$$

its first term given in Mr. Reid's paper in equation (16.)

If there are no precautions taken against the fluctuations of

the field current, angle α approximates 0; and cos α therefore $= 1$.

On the other hand, the influence of the second term, the fluctuations of the exciting current, can be decreased by increasing angle α, therefore decreasing cos α; that is, by throwing self-induction in the exciting circuit.

This latter phenomenon I had occasion to observe some time ago, the potential of an alternator, which I tested, being raised from 22 volts to 160 volts by means of two impedance coils (transformers with open secondary), connected in the exciting circuit.

An approximate calculation shows that, although the term of magnetic saturation, the third term, in equations (16) to (19), is small enough to be neglected in most cases, the second term, which accounts for the influence of the fluctuations of the exciting current, can not well be neglected.

Even the equations given above can hardly be considered as an absolutely complete solution of the problem of armature reactions of alternators, because they still neglect terms of tertiary order.

But they will be found to hold for almost all the types of modern alternators, and only in those few machines, where the fluctuations of the field current are unusually heavy, (especially where the electric induction is produced by changes of the magnetic resistance, and field magnetism and armature magnetism are connected in series in the magnetic circuit), do those fluctuations become so important, that a more particular calculation of them, including the consideration of the self-induction in the field circuit, has to be made. Into this, however, I do not need to enter deeper, because such machines, although they were operated many years ago (Klimenko's alternator, for instance, tested at the Electrical Exhibition in Vienna, 1883), have not come into more extensive commercial use.

Discussion.

Mr. Thorburn Reid:—Mr. Chairman: It has given me great pleasure to find that Mr. Steinmetz has been able to make use of my equations to carry the analysis on to a further degree of approximation. I have not attempted to apply them to any machine except that of Mr. Stanley's, described at our last meeting, and as I found them amply sufficient for that purpose, I did not go any further. Having accomplished the purpose with which I started the analysis, I laid it aside to go into other matters requiring more immediate attention. Mr. Steinmetz's approximation would, I have no doubt, be of use in some forms of alternators with which I have not come in contact and I see that he mentions one case where they would have been immediately applicable. Now in regard to the application of the method to the Stanley arc machine, I saw the difficulties he mentioned before I finished the analysis. But as I mentioned at the last meeting, considered them of small enough value to be neglected. The large variation of the field current shown by Messrs. Tobey and Walbridge's paper rather staggered me at first, but on thinking over the matter I came to the conclusion that that fluctuation is not as great as it appears at first sight. Mr. Steinmetz remarks that the *magnetism* varies more than 50 per cent., but if he will consider the matter he will see that not the magnetism but the *field current* varies 50 per cent. Now those of you who are familiar with Ewing's curves of hysteresis will remember that he forms some loops in those curves by stopping the current at some point and going back on its tracks a little way, returning to his starting point, and if you remember the form and position of those loops you will see that they show the magnetism as varying very little with an enormous variation in the E. M. F. and especially is this the case where the saturation is high as it is in Mr. Stanley's alternator. Thus a very small variation in the magnetism will account for a very large variation in the current. Therefore the constant a equation (8) is likely to be very much smaller than would appear at first sight. Further, a reference to the curves in Messrs. Tobey and Walbridge's paper shows that the lag of the field current varies only a few degrees from short circuit to full load and that its maximum almost exactly coincides with that of the armature current. Therefore the value of the magnetic flux at the instant when the armature pole is opposite the field pole must vary very little since the instantaneous values at the maximum point are varying very slowly. Taking all these points into consideration then it appears to me that the influence of this term will be vanishingly small and even if it were not so, there are so many other variations at least as large affecting the result that the addition to this term would be of doubtful utility for practical purposes and even for theoretical, unless other variations are also allowed for.

The third term in Mr. Steinmetz's equation (19), he appar-

ently does not consider of importance nor do I. Not, however, because it is small, though it may be as far as I know, but because its effects would be impossible of prediction in view of the other variables of which no account has been taken as yet, and which I will now mention.

From Mr. Steinmetz's second equation, I judge he must have been mistaken in the meaning that I gave to ρ in my paper or else overlooked the importance of some terms that are left out of that equation, supposing them to be constant. ρ is not equal to the reciprocal of μ but to μ multiplied by L divided by A. Both L and A are variable during the revolution of the armature and their variation is probably much greater than that of μ. Of course the variation will have to be added to that of the magnetic circuit; and right here another complication will step in, namely the fact that, as resistance is added in the main circuit more lines will go through the iron inside the coil and less through the iron around the coil. The resistance of these two parts of the magnetic circuit will thus be changed to a considerable extent. In other words, μ will change not only during a revolution of the armature, but also while the resistance is being changed in the main circuit. One more complication that has not been considered and I will stop, though I might mention others. The fluctuation of the field current is affected by the various varying values of μ, by the change in the magnetic resistance due to rotation of the armature and the eddy currents which must be considerable, since the field is not laminated. There is every reason to believe then that in Mr. Stanley's machine at least, the terms introduced in Mr. Steinmetz's equation will not materially help us, in view of the number of other terms, equally as important which would have to be introduced to completely solve the problem. As I have said before, the equation I obtained, agrees nearly enough with the actual performance of the machine for all practical purposes and furnishes a means of designing the machine intelligently. In applying these equations, I had to do it by means of the curve of current and electro-motive force obtained in practice. There was no way that I could get up, of measuring the length and cross-section of the magnetic circuit of the armature. I obtained the value of ρ by means of one point on the curve of current and E. M. F. which is obtained by practice, and substituted in the equation. Now that is a very rough method, but what I discovered was that my equation agreed with the curve, or rather the curve was very nearly expressed by my equation, so that it appears to me that the equation was close enough for all practical purposes.

I hope neither Mr. Steinmetz nor any one present will think that I am underrating the importance of the equation which he has evolved in so masterly a manner. He has had experience with machines which I have never been called on to analyze, and the terms that I have neglected may be of great importance in

those machines and the other difficulties may be so small as to not materially affect the result. In that case his equation will, no doubt, be of great value and in any case it is important as throwing more light on a very difficult and complicated analysis and some one who has more time and a better knowledge of mathematical processes than I have, may be Mr. Steinmetz himself, may be able to take in the other variations and obtain a complete analysis of the machine. I would like to suggest to Mr. Steinmetz the question as to whether it would not throw more light on the matter to carry out his sine series to a further point in view of the many variables that affect the final result. This I am afraid, however, would lead to so complicated an equation that it would be impossible to determine the constants, not to speak of the integration. Some of the higher orders may be of more importance also than those included.

MR. STEINMETZ:—I can hardly remember all the remarks Mr. Reid offered here, and can answer therefore only a few, which I just remember.

In regard to the term μ in equation (2), I believe Mr. Reid misunderstood me. I introduced this μ only for convenience's sake, as the inverse value of the term ρ, which Mr. Reid used; but this μ has nothing do do with the "magnetic susceptibility" or "specific magnetic" conductivity, as follows from the equation (3).

The eddy-currents induced in the field iron of the machine, and the hysteretic lag are accounted for already in my equations by the angle of lag of field magnetism a, and its amplitude G.

Furthermore, these constants F, G, a, λ, ϑ, can very well and easily be calculated, although I did not go further into the consideration of these terms, because I intended to give only a short note on this analytical method.

If I proposed to determine these constants, I would try the following way:

The terms F, G, a are very much independent of the armature circuit, and depend especially upon two factors:

1. The shape of the iron circuit of the machine.

2. The self-induction of the field circuit, and, if this is not laminated, but an ordinary cast-iron field, the amount of eddies induced therein.

Only a certain part of the lines of magnetic force, induced by the armature current, will go through the magnetic field circuit, and this part will be greater or smaller, according to the shape of the magnetic field circuit; for instance pretty small for a smooth armature or an armature with wide overlapping teeth, still smaller for a disc armature and long, highly saturated field magnets, larger in those fly-wheel types of alternators very often proposed and hardly ever used.

Let δ = this percentage of armature lines going through the field circuit.

In the alternator which I referred to at the last meeting, this percentage was 1, that is, all the armature lines went through the field circuit; the machine was intentionally built so, for carrying out some experimental researches. This feature explains the unusually heavy fluctuations of the field current in this particular machine.

Then the whole number of armature lines going through the field circuit will be:

$$M^1 = \delta S \mu i \cos \varphi$$

and the number of lines produced by the field exciting current proper therefore:

$$N - M^1 = F - G \cos (2 \varphi - \alpha) - \delta S \mu i \cos \varphi$$

where for the current only the first term of equation (13) is sufficiently exact:

$$i = \frac{2 \pi n F S}{R \sqrt{1 + \frac{4 \pi^2 n^2 \sigma^2 S^4}{R^2}}} \sin (\varphi - \varepsilon)$$

Now c being the field exciting current, S^1 the number of field turns, μ^1 the magnetic conductivity of the field circuit, the number of lines induced by the field current is:

$$= S^1 \mu^1 c$$

and, this being $= N - M^1$

gives us the differential equation for the determination of F, G, α which differential equation still contains the term c, the field current:

This field current is obtained in the following manner:

May E denote the E. M. F. of the exciter dynamo, r the field resistance. The E. M. F. induced in the field circuit by the fluctuations of the magnetism is then:

$$e = - S^1 \frac{d N}{d t} = - \frac{2 S^1 G}{s} \sin (2 \varphi - \alpha)$$

and the field current is:

$$c = \frac{E - \frac{2 S G}{s} \cos \omega \sin (2 \varphi - \alpha - \omega^1)}{r}$$

where ω and ω^1 give the shifting of phase and the decrease of amplitude, caused by self-induction and by the eddy-currents in the field iron, calculated by the well-known equations of inductive resistances and transformers (the eddy-currents taking the place of the secondary current in the transformer, for no eddies $w = \omega^1$).

This value c substituted in the equation derived before:

$$N - M^1 = S^1 \mu^1 c$$

gives us a simple differential equation in 2φ, which can be integrated very easily, and gives the proper values F, G, a.

In regard to the periodic function μ, the amplitude λ is the decrease of magnetic conductivity for the maximum saturation used in the alternator, and to find the shifting of phase θ, we may assume that the minimum of μ occurs together with the maximum of current i, with a small advance of phase ψ due to hysteresis, (so that the minimum of μ takes place a little before the current i reached its maximum.)

Then we have $2\varphi - \theta = O$ for: $\varphi - \varepsilon = 90° - \omega$ and therefore: $\theta = 180° + 2\varepsilon - 2\psi$

or: $\theta = 2\varepsilon - 2\psi - 180°$

DR. PUPIN:—I would like to make a very short remark only. Mr. Steinmetz obtained the wave of the current to be composed of several waves. Some having the period of the alternator and others having a triple period; besides, the waves differ in phase. That would mean that the resultant wave would have a larger lag than found experimentally, and it would also mean that the wave which they obtained experimentally would not be a simple harmonic wave, but would have kinks in it. It follows from the fundamental differential equation for the currents in an alternator that as soon as we have two periodic quantities in it besides the current—that the solution cannot give us a periodic function which is a simple harmonic. We have seen that as soon as an arc-lamp is introduced into the circuit, there is introduced besides the variable coefficient of mutual induction N, another simple harmonic variable which is the resistance of the total circuit, and we obtain, as was shown experimentally, a complex harmonic for the wave. In the case, however, where we have incandescent lamps in the circuit a simple harmonic wave is obtained for the current and E. M. F. It follows therefore that every integral which gives us anything but the simple harmonic for an incandescent circuit disagrees with the experimental facts.

I find that if we introduce N as a simple harmonic function, but let ρ remain constant we really obtained a simple harmonic wave, which is in perfect agreement with the experimental facts, and a short consideration shows that ρ is really a constant. ρ depends on two things; first, on the permeability of the iron in the armature, and secondly on the permeability of the iron in the field magnets. Now if the saturation of the iron in the armature is not carried beyond nine or ten thousand lines of force to the square centimetre, then the coefficient of permeability remains practically constant, as we know from Ewing's curves. Now I was told by Mr. Reid at the last meeting that the magnetization of the iron in the armature was not carried beyond nine or ten thousand lines of force per square centimetre. Therefore, as far as the armature is concerned, the quantity ρ in Mr. Reid's paper

or μ in Mr. Steinmetz's paper is a constant quantity. Next we have to examine what influence has the iron in the field magnets upon the coefficient of permeability of the armature circuit? The iron in the field magnets is magnetized to a pretty high degree of saturation; perhaps fourteen thousand, perhaps twenty thousand lines of force to the square centimetre. The armature current acts always in such a way as to tend to demagnetize the iron in the field magnet. Now we know when iron is highly magnetized, if a force be applied in the opposite direction tending to demagnetize it, that the resistance of the iron to the second force is almost as great as the resistance of air. I heard this fact first mentioned by Mr. Eickemeyer of Yonkers, a man who has large practical experience in these matters, and I examined that point also from Ewing's curves. It really follows, when iron is magnetized to a considerable degree of saturation, that if we apply a force in the opposite direction to demagnetize it, that for forces smaller than the force which produced the first magnetization, the coefficient of permeability is almost as small as that for pure air. Therefore as far as the magneto-motive force of the armature current is concerned, the magnetic resistance of the field magnet is the same as that of air. Therefore the magnetic resistance opposed to the magneto motive force of the armature current depends only on the condition of the iron in the armature and therefore as long as that iron is not magnetized above the ten thousand lines of force to the square centimetre, the magnetic resistance opposed to the current in the armature is constant throughout, and if we make that supposition in our differential equation and substitute for N the induction due to the field magnet, a simple harmonic function as I suggested at the last meeting, (the conditions of our problem suggest a simple harmonic function for it) if we make that substitution for N, we get a solution exactly the same as Mr. Reid's, only the constants will differ somewhat; the wave of the current will be a simple harmonic wave, and that is just what the experiment found it to be.

MR. STEINMETZ:—I made the same observation, that by assuming only N as variable, the differential equation of the alternator gives a finite integral.

But, having spoken of the variability of the term ρ in the last meeting, I thought it my duty to get the variability of μ into the equations too, to make them complete, although a numerical calculation showed me that at least in the alternator tested by Messrs. Tobey and Walbridge this third term is rather small and unimportant, as I already stated in my paper, so that it does not show up in the current curves

I just now remember another remark in Mr. Reid's answer, about the decrease of the fluctuations of field magnetism due to hysteresis. It is not hysteresis alone, which causes a discrepancy between field magnetism and field exciting current, but those

considerations given above for calculation of the field constants F, G, a, etc., show that the field magnetism does not go together with the exciting current, but differs in phase, and has a smaller amplitude than the current, because of the eddy currents and of the "back-induction" to use this improper term, from the armature circuit to the field.

Adjourned.

American Institute of Electrical Engineers.

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PERRINE, FREDERIC A. C.	Superintendent Insulated Wire Dep't, John A. Roebling's Sons Co., 137 E. State St., Trenton, N. J.	Joseph Wetzler. T. C. Martin. R. W. Pope.
MERRITT, ERNEST	Instructor in Physics, Cornell University, Ithaca, N. Y.	E. L. Nichols. H. J. Ryan. W. A. Anthony.
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Total 18.

Council Meeting, October 21st, 1890.

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MARTIN, J.	Electrician, Hanson, Van Winkle & Co., 16 Oak St., Newark, N. J.	Jos. Wetzler. T. C. Martin. R. W. Pope.
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SCHMID, ALBERT	Superintendent, Westinghouse Electric & Mfg. Co., Pittsburgh, Pa.	O. B. Shallenberger. Nikola Tesla. Jos. Wetzler.
SHEBLE, FRANKLIN	Electrical Engineer, Thomson-Houston Electric Co., Lynn, Mass.	Jas. B. Cahoon. E. W. Rice, Jr. A. L. Rohrer.
WATSON, ROBERT	The Elektron M'f'g Co., 79 Washington St., Brooklyn, N. Y.	T. C. Martin. Jos. Wetzler. Geo. M. Phelps.
WEBB, HERBERT LAWS	Assistant Electrician, The Metropolitan Telephone and Telegraph Co.. 18 Cortlandt, and 159 W. 94th St., N. Y. City	Geo. A. Hamilton. T. C. Martin. Geo. B. Prescott, Jr.
WELLER, HARRY W.	Railroad Inspector, The Edison General Electric Co., Edison Building, Broad Street, New York City, and Jamaica, N. Y.	J. H. Vail. D. C. Jackson. Jos. Wetzler.
WRIGHT, JOHN D.	Electrician's Assistant, United Electric Traction Co., 107 Garrison Ave., Jersey City, N. J.	Jos. Wetzler. Geo. M. Phelps. R. W. Pope.

Total 19.

Council Meeting. November 18th, 1890.

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DONNER, WILLIAM H.	Designing Draughtsman, and Electrical Engineer, Thomson-Houston Electric Co., 113 Franklin St. Lynn, Mass.	C. J. Van Depoele. A. L. Rohrer. Edward Weston.
LOGAN, CHARLES H.	General Manager, Standard Electric Co., Detroit, Mich.	W. J. Hammer. Geo. M. Phelps. Joseph Wetzler.
OTTEN, DR. JAN D.	Engineer, Thomson-Houston International Electric Co., Michaelsbrucke 1, Hamburg, Germany.	A. L. Rohrer. E. W. Rice, Jr. Louis J. Magee.

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Council Meeting, December 16th, 1890.

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DRESSLER, CHARLES E.	Maker of Scientific and Electrical Apparatus, College of the City of New York, 17 Lexington Ave. New York City.	Alfred G. Compton. Chas. A. Doremus. H. A. Foster.

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FRENCH, E. L.	Draughtsman, Mather Electric Co., Manchester, Conn.	W. A. Anthony. D. C. Jackson. David E. Lain.
HANDLEY, ARTHUR	Electrical Engineer, Electrical Engineering Co. of Ireland, L't'd, 61 Dawson St., Dublin, Ireland.	R. W. Pope. T. C. Martin. Jos. Wetzler.
MAYER, GEORGE	Mechanical Draughtsman, Edison General Electric Co., 68 Broad St., New York City.	A. E. Kennelly. A. E. Winchester. J. H. Vail.
MURPHY, WM. F.	Electrician, Spartanburg Electric Light and Power Co., Spartanburg, S. C.	R. W. Pope. Jos. Wetzler. T. C. Martin.
PAINE, F. B. H.	Consulting Electrical Engineer, Chas. Paine & Sons, 71 Broadway, New York City.	Charles A. Terry. H. M. Byllesby. Albert Schmid.
POOR, CHAS. LANE	Tutor, College of the City of New York, 17 Lexington Ave., New York City.	Alfred G. Compton. Chas. A. Doremus. Francis B. Crocker.
SHELDON, SAMUEL, *A. M. Ph. D.*,	Professor of Physics and Electrical Engineering, Polytechnic Institute, Brooklyn, N. Y., Residence, 20 Sidney Pl.	Geo. M. Phelps. James Hamblet. T. C. Martin.
SPRUSON, WILFRED J.	Member of the firm of Hepburn & Spruson, Consulting Engineers and Electricians, 169 King St., Sydney, Australia.	W. J. Johnston. Louis Bell. Clarence E. Stump.
SULLIVAN, M. C.	With the *Electrical Engineer*, 150 Broadway, New York City.	Jos. Wetzler. T. C. Martin. R. W. Pope.
WARING, JOHN	Consulting Electrician, The Perkins Electric Lamp Co., Manchester, Conn.	W. A. Anthony. D. C. Jackson. David E. Lain.

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Approved by Board of Examiners, December 3, 1890, Confirmed by Council December 16, 1890.

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DONNER, WILLIAM HERBERT	Designing Draughtsman and Electrical Engineer, T-H Electric Co., Lynn, Mass.	Nov. 18, 1890.
FESSENDEN, REGINALD A.	Assistant Electrician, U. S. Electric Light Co, Newark, N. J.	Oct. 21, 1890.
HUTCHINSON, DR. CARY T.	Electrical Engineer, 56 West 25th St., N. Y.	Feb. 7th, 1890
PERRINE, DR. FREDERIC A. C.	Supt. Insulated Wire Dept., Roebling's Co., Trenton, N. J.	Sept. 16th, 1890
WEBB, HERBERT LAWS	Asst. Electrician, Metropolitan Telephone and Telegraph Co., New York.	Oct. 21st, 1890
WOLCOTT, TOWNSEND	Consulting Electrician, 849 Greene Ave., Brooklyn, N. Y.	Mar. 6th, 1888.

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